DOCUMENT RESUME

ED 200 490 SO 013 255

AUTHOR Shaver, James P.
TITLE What Can Research Contribute to Law-Related Education?
PUB DATE Oct 80
NOTE 22p.; Paper prepared for an American Bar Association Conference on Law-related Education Futures (Aspen, CO, October 12-15, 1980).

EDRS PRICE MF01/PC01 Plus Postage.
DESCRIPTORS *Educational Research; Elementary Secondary Education; *Legal Education; *Research Needs; *Research Problems; Social Studies; State of the Art Reviews

ABSTRACT

Research in law-related education has not led to cumulative knowledge that is of much use to curriculum developers or classroom teachers. Among the reasons for this state of affairs are that researchers concentrate on nonessential topics, educational research does not generally meet criteria for scientific research, educational research is too often conducted in isolation from prior research and theory. Also, researchers often lack sensitivity to the realities of the classroom due, at least in part, to their failure to involve teachers in planning, conducting, and interpreting research. Ways in which research in law-related education can overcome these problems include that researchers should show more respect for empirical research and previous research findings, that educators should become more realistic with regard to their expectations for educational research, and that researchers pay more attention to developing explanatory concepts and to integrating and interpreting research literature. Major activities in law-related education in the 1980's will more than likely center on developing, implementing, and institutionalizing educational programs in schools and other institutions. (DB)

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WHAT CAN RESEARCH CONTRIBUTE TO LAW-RELATED EDUCATION?*

James P. Shaver

UTAH STATE UNIVERSITY

October 1980

SP 013 255

WHAT CAN RESEARCH CONTRIBUTE TO LAW-RELATED EDUCATION?*

James P. Shaver
Utah State University

Rationale

During the last three or four decades, the gains in knowledge from research in the physical and biological sciences and the results of the technological applications of that knowledge have been publicly visible and impressive. It is not surprising, in that light, that many professional educators have sustained their faith that educational research would also provide a firm base of knowledge from which to make curricular and instructional decisions. As Schutz (1979) has noted, there has been a persistent belief that research results could be "converted into educational practice". Disagreement has not been over the potential contributions of more research, but whether that research should be "applied" or "basic" (p. 6). Pillemer and Light (1980) also allude to the enduring motivation to do more educational research, because of the assumption that "as more information . . . accumulates, and as our information-gathering techniques become more sophisticated, researchers will be better able to zero in on the correct answer or answers . . . and knowledge [will converge] upon an underlying truth" (pp. 177, 193).

This drive toward increasing the volume of research while also increasing the sophistication of statistical tools has been prevalent in educational research in recent years. There are, however, signs that inferential statistics is losing its hold on the educational research community (Rist, 1980). Whether other research strategies now becoming

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*Paper prepared for an American Bar Association Conference on Law-related Education Futures, Aspen, CO, Oct. 12-15, 1980.

fashionable, such as ethnography (Smith, 1978), will be any more productive of findings useful to practicing elementary and secondary school educators remains to be seen. However, past experience does not suggest a very optimistic prognosis.

Serious questions can, and have been, raised about the overall productivity of educational research to date. For example, Shaver (1979b) claimed that

> there would have been little discernable effect on educational practice if most of the studies reported in educational journals and dissertations had never been conducted. (p. 3)[1]

It has been asserted (Shaver, 1979c) that in social studies, an area closely related to law-related education (some would argue, encompassing law-related education), the situation is similar to that for education generally: Research has not led to cumulative knowledge that is of much use to those who must make curricular and instructional decisions in the schools (Shaver, 1979c). All of this is not to say that educational research has had no impact on practice. As Clifford (1973) has noted, studies--not necessarily the least deficient or most sophisticated in design and execution--have occasionally influenced schooling. Generally, however, educational research has not been continued over the years because of its clear contributions to educational practice. A more likely reason is its status as an activity expected of those who obtain graduate degrees (Clifford, 1973, pp. 2, 28).

Many reasons have been suggested for the lack of productivity of educational research. A fundamental criticism is that despite much talk about scientific educational research (see, e.g., Kerlinger, 1977),

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[1] See Clifford (1973, p. 23) for a similar view.

little understanding of science is evidenced by those who do educational research (Shaver, 1979b). The emphasis has been on discrete, one-shot studies, with an over-emphasis on the use of inferential statistics (Carver, 1978) and an under-emphasis on the replication of findings (Shulman, 1970; Campbell & Jackson, 1979). Even if one were to reject Carver's (1978) claim that the reliance on statistical tests of significance is a perversion of science, the fact is that little educational research, including that in social studies (Shaver & Norton, 1980a), meets the assumptions of random sampling and/or assignment that underlie the theoretical sampling distributions used in statistical inference (Shaver & Norton, 1980b).

It has also been suggested that educational research is too often conducted in isolation from prior research and theory. Clifford (1973, p. 35) has suggested that the typical reviews of the literature included in dissertations are "travesties" in that they provide no real linkage with past research or theory. The lack of theory-based research in social studies has also been attacked (Metcalf, 1963; Shaver & Larkins, 1973). The conducting of series of theory-related critical experiments (Platt, 1964), each of which will move the researcher closer to the understanding of the phenomenon under investigation, is practically unheard of in education.

Lack of sensitivity to the realities of social studies classrooms, in part due to the failure to involve teachers in planning, conducting, and interpreting research, has also been cited as a reason for the lack of usefulness of educational research, especially in social studies (Shaver, 1979c). And Schutz (1979) has proposed that much of the unproductiveness of educational research efforts can be traced to the untenable assumption, noted above, that "research can be directly converted

into educational practice". That aim, he claims, is "akin to alchemy" (p. 6). But the problem may run deeper than misunderstanding of science, the use of inappropriate paradigms, or failure to address what really happens in school.

It may indeed be that those who study human behavior cannot realistically hope to attain the same goals as those in the physical and biological sciences: The amassing of empirical generalizations, the restructuring of the generalizations into more general laws, and the welding of scattered laws into coherent theory (Cronbach, 1975). Among others, Cameron (1963) has argued that humans are too variable and reactive to experimental conditions for the valid use of what have traditionally been regarded as scientific methods of research. Gergen (1973), in a paper that initially sparked considerable controversy[2] but since has been largely ignored, argued that the success of the physical sciences could not be expected in the study of human behavior because continuing cultural changes soon invalidate generalizations, and because the dissemination of knowledge gained from research itself changes the patterns of behavior studied. Building on Gergen's paper, Cronbach noted that while "the physical theorist can regard the processes in his world as steady, [r]arely is a social or behavioral phenomenon isolated enough to have this steady-process property." One should not draw the inference that the problem is that "human events are in principle unlawful," because "man and his creations are part of the natural world". Instead, the difficulty is that "we cannot store up generalizations and constructs for ultimate assembly into a network" (p. 123, italics his).

Along with shifts in human behavior patterns over time, the variability of humans creates special problems for educational researchers.

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[2]See Schlenker (1974) and Volume 2 (1976) of the Personality and Social Psychology Bulletin, pp. 371-465.

Individuals react differently to the same situations, so that curricula and instructional techniques rarely seem to have uniform effects across individuals. In other words, treatments interact with a number of population factors, including student characteristics and teacher characteristics, and ecological factors such as the subject matter being taught, the nature of the school setting, the local community, and contemporary history. Snow (1977) has indicated that the perplexity for those who want to apply research findings is that "an instructional treatment that is best on the average may still serve some students poorly" (p. 13). Martorella (1977), in a review of social studies research, made the point even more emphatically: "[W]hile research apparently continues to add to our knowledge about individuals in general, it tells us nothing about any given individual. It may even distort our perspective on a particular student" (p. 44).

Cronbach (1975) referred to the interactions between treatment and other factors as "a hall of mirrors that extends to infinity". For, as he put it, "however far we carry our analysis . . . untested interactions . . . can be envisioned" (p. 119). On that basis, Snow (1977) maintained that "general instructional theory . . . is a holy grail" (p. 15). If there is to be instructional theory at all, he argued, it must be local--specific to particular settings, subject matter, students, and teachers--as well as time-limited, and likely never "sufficiently explanatory for scientific taste" (p. 15).

Clearly, then, there is considerable reason to question the role of research in understanding human behavior, in comprehending education in general, and in assisting in curricular/instructional decisions in social studies (and, therefore, law-related education) in particular. Again,

it is not that research has made no contributions to education (Clifford, 1973), but that it has never come close to fulfilling the optimistic expectations for it. Perhaps part of the disappointment is due to the failure of educators to really believe in and act upon theoretical formulations, such as that of John Dewey, which might have sufficient strength to have widespread validity (i.e., across teachers, students, settings) and to serve productively as a basis for related, cumulative research (see, e.g., Metcalf, 1963; Shaver & Larkins, 1973; Shaver, 1979a). But even if one accepts that attempts to apply "scientific" epistemology to educational problems must be largely and inherently abortive because humans are so different from the subject matter of the physical and biological sciences, does that mean that educational research has no role?

Both Gergen (1973) and Cronbach (1975) think not. Cronbach cited two contributions that research can make: It can help us "to assess local events accurately in order to improve short-run control", and it can help us "to develop explanatory concepts, concepts that will help people use their heads" (p. 126). The latter suggestion is akin to Shaver's (1979c) conclusion that

> the greatest benefits [to social studies personnel] from the research literature will be heuristic in nature. Research reports may stimulate thinking about instructional and curricular alternatives. . . ." (p. 41)

Gergen (1973), too, saw an important role for research

> as a sensitizing device . . . [to] enlighten one as to the range of factors potentially influencing behavior under various conditions . . . [and providing] some estimate of the importance of those factors at a given time. (p. 317)

Gergen (1973, p. 318) also proposed that research into the "relative durability of social phenomena", especially through replication, could

make significant contributions.

Snow (1977) also would not discontinue educational research, only have it set in a realistic context. An important role of research could be to assist in the development of local instructional theory and to determine the applicability of such theories to other populations and settings. Schutz (1979) took a similar position. We should eschew the fiction that "technology is science applied" and engage in R&D geared to the specific instructional concerns of school personnel, utilizing our technological knowledge and the base of concern and competence now available in schools. His proposal does not imply the abandonment of educational research that is not in the R&D mode. Rather, the products developed through competent R&D should enhance research by insuring against studies that investigate "vacuous and trivial treatments" (p. 7). The point is that what is needed is not more and more research with the vague hope that cumulative, applicable findings will be the result, but research that is conditioned by careful thought about the realities of human behavior in general and schooling in particular, and about appropriate research strategies.

Current Developments

Law-related education is a fairly recent rubric, so one would not expect to find much educational research published under that label. Such is the case. However, because of the citizenship orientation of social studies (Shaver, 1967, in press), much research in that area should be relevant to law-related education--at least according to the Law-Related Education Act of 1978, which defines law-related education as:

> education to equip nonlawyers with knowledge and skills pertaining to the law, the legal process, and the legal system and the fundamental principles and values on which these are based. The purpose is . . . more informed and effective citizens (Fed. Reg., 1980 [Apr. 24], 45[No. 81], p. 27880).

Some studies, such as the research of Oliver and Shaver (1974) in the late 1950's based on a "jurisprudential model" for social studies, are clearly law-related in their intent.

The status of research knowledge in social studies is, then, relevant to law-related education and was referred to earlier in this paper. There probably is little need to reiterate the rather grave diagnosis presented there. Conclusions as to the dire state of the field have been supported by Wiley's (1977) review of reviews of research and by two recent sets of reviews of research (Hunkins, 1977; Leming, 1979). McPhie (1979) encapsulated the situation as follows:

> . . . [E]ven though there have been literally tons of research reports submitted in social studies education, the studies to date have been so segmented and unrelated and the results have been so contradictory that comparatively little has emerged which can be used with confidence by those who make decisions about curriculum and instruction. (p. 588)

Wiley (1977) was somewhat optimistic about the research in two areas: Teaching for critical thinking (pp. 174-7)--despite characterizing that area as one of "few scattered findings, which are tentative at best . . ." (p. 176); and in regard to "models for factual and conceptual teaching" (p. 192)--despite the conclusion of Martorella (1977) (whom Wiley cites), that "there are still more questions than answers" about the "instructional variables that have a significant effect upon cognitive outcomes" (p. 45). Nevertheless, she lamented the "lack of a cumulative research base in social studies/social science education" (p. 165), the

lack of research "focused on questions about the relative merits of different kinds of content (e.g., social science, public issues, chronological history) in achieving the goals of social studies" (p. 162), and the lack of "research on the effectiveness of various types of curriculum materials" (such as textbooks). And as for the effectiveness and efficiency of instructional methods and techniques, a topic which accounts for a "large portion" of the research, Wiley concluded:

> This area . . . appears to be fairly chaotic . . . [and] to have yielded few conclusions that one can endorse with much confidence and few guidelines for practitioners. (p. 28)

The litany could be continued if the point were simply to disparage past research efforts. The purpose is rather to indicate the state of the field as an impetus for considering what types of efforts should be encouraged in the future.

Needs

Many of the needs for research in law-related education are evident from the preceding discussion. Fortunately, because law-related education is a relatively new field which is bringing together people from a variety of backgrounds, there is opportunity for examination of the question whether research has a contribution to make--or better put, how can research contribute to law-related education? The brief, non-inclusive listing of needs which follows is premised on the validity of that question, and on the possibility that it can be examined rationally and openly because of the diversity of perspectives present among law-related educators.

I have raised serious questions about the attainability of theory in education in the sense of the laws of physical science. However,

educational research can, I believe, be scientific in the sense of adopting the basic epistemological assumption of respect for empirical, replicated findings. In one of his usually insightful statements, Boulding (1980) rejected the frequently elaborated dichotomy between "hard" and "soft" science, and suggested that, instead, it is more appropriate to draw a distinction between "the 'more secure' and the 'less secure' sections of the whole sphere of human knowledge". Fields of knowledge in which "the available data only cover a small part of the total field" and in which "the actual structures and relationships . . . are extremely complex . . . [e.g.,] our knowledge of individual human behavior" are likely to be insecure, as are fields that do not study events that are "common and repeatable-at-will" (p. 834). Surely, education and psychology are insecure fields (see, e.g., Gergen, 1973, and Cronbach, 1975). Boulding (1980) also noted the "great variety of methods . . . within the scientific community" and the "handicapps to the growth of knowledge" that result from the uncritical acceptance of methods from other fields. In fact, he claimed:

> . . . [O]ne of the problems which science still has to face is the development of appropriate methods corresponding to different epistemological fields. (p. 833)

It is probably fruitless to argue whether educational research is science, insecure or not. It does seem clear, however, that a major problem in education in general, and law-related education in particular, is the development of research paradigms--strategies, approaches, methods--appropriate to an epistemology grounded in the nature of human, not physical or lower biological, phenomena. In particular, the dominant but unproductive and ill-applied, model of single, discrete studies

with quantitative data analyzed using inferential statistics should be largely abandoned (Shaver, 1980).

At the same time, there is a need to build realistic expectations for educational research, without making the trivial acceptable. Gergen (1973) and Cronbach (1975), for example, suggest that we can reasonably expect educational research to provide school people with assistance in "using their heads", not to produce principles to be welded into general theories. Their position is a provocative place from which to initiate thought about models of research appropriate to law-related education, and from which to encourage realistic research--research done from a model that includes teachers as full partners, that encourages "local" studies along with replications across populations and sites in the hopes of finding continuities. One aspect of this strategy would be the encouragement of careful R&D (Schutz, 1979) to meet "local" needs, accompanied by research to determine the applicability of the products elsewhere, and use of the products in research to extend our knowledge of the constraints and opportunities in law-related education. Clearly, in this model, evaluation--the subject of another paper--would not be viewed as a separate endeavor. Although evaluation would still have as a central thrust the answering of important program-oriented questions, it would also, to avoid being wasteful of time and effort, incorporate an "outward" orientation toward providing others with data to use in "using their heads" about program decisions.

There are dangers to such an approach. Too narrow a focus on the local and the specific and an attendant rejection of the notion of research expertise could lead to a debacle similar to that of "action research" in the 1960's (Clifford, 1973, p. 21). The argument is not that research

in law-related education simply be left to local school and project personnel, but that it be guided by an epistemology appropriate to the problems of generalization in education.

By the same token, identification of the need to reconceptualize our approach to educational research should not be taken as antitheoretical, although there is in the preceding pages considerable skepticism about the likelihood that theory as it is known in the physical and biological sciences can be built in education. We must still entertain the possibility that, even accepting the proposition that the tightly coherent and predictive theories of the physical sciences can probably not be achieved, there may be a theory base that could provide conceptual unity and thrust to law-related education efforts and be powerful enough to be applicable across populations, sites, and time--perhaps even providing a basis for the crucial, hypothesis-excluding experiments which Platt (1964) advocated.

Metcalf (1963) proposed that the theoretical work of John Dewey could provide such a foundation, and I am inclined to agree. My enthusiasm for Dewey's straightforward notions, for example, that thinking and learning cannot be separated and that we think about those things that pose problems that are real to us as individuals, was reconfirmed by Curtis' (Curtis & Shaver, 1980) recent dissertation research with low academic students. His results suggest that if involved in problems that they see as influencing their lives (and the relevance may need to be illustrated, as Curtis did with housing shortage and expense in British Columbia), these students can and will learn much that is ordinarily deemed to be beyond them.

Interestingly, a great deal of the work in law-related education has a Deweyean flavor, although I suspect that is largely unintentional. There has been a striking emphasis on student activity centered around issues that matter to young people and concepts that will help them to construe and control their own lives. Much of this is John Dewey at his best (Shaver, 1977). The reasons for this emphasis among law-related educators are not clear to me; but I suspect that a major factor is that many of the programs have arisen not only from a concern, often by those not already a part of the educational establishment, with helping young people to comprehend the law as a vital part of their own lives, but in reaction to traditional schooling, including social studies, which is about as nonDeweyean as could be (see, e.g., Shaver, Davis, & Helburn, 1978). To capitalize on potential relationships to theory seems to me to be a prime need if law-related education is to realize its full potential and not become unduly compromised by the "real demands" of schools and schooling.

One further need merits attention: Despite the rather gloomy diagnosis of the state of educational research presented earlier in this paper, it is important to consider the possibility that there may be more knowledge residing in the thousands of studies reported in the research literature than scholars have been able to glean using the selective, impressionist review techniques that have been popular (Jackson, 1978; Glass, 1976; Gage, 1978). The integration and interpretation of the research literature deserves more attention than it receives (Feldman, 1971). While there are reasons not to be overly optimistic about the potential results (Eysenck, 1978; Gallo, 1978; Mansfield & Busse, 1977; Shaver, 1979c, pp. 29-30), techniques for

synthesizing research findings, such as Glass' (1977; also see Pillemer & Light, 1980) meta-analysis, ought to be tried.

All in all, the critical re-examination of the typical educational research strategies and methods as these apply to law-related education is the major challenge that comes from consideration of the current state of research knowledge.

The Next Five to Ten Years

Undoubtedly, the major activities in law-related education in the next five to ten years will center on the development, implementation, and institutionalization of educational programs in schools and other institutions. Little money is now explicitly available for research, and that is not likely to change. However, the emphasis on program installation does not mean that research should be ignored. Those in law-related education--in particular, perhaps, the American Bar Association's Special Committee on Youth Education for Citizenship (YEFC), should encourage the incorporation of research in development-installation projects whenever possible.

In some instances, the inclusion of research may be no more than adoption of a more "outward" perspective in project evaluation efforts, focusing more on providing evaluation data that will assist those interested in adopting, or adapting, programs at other sites. On a broader scale, greater attention to the Department of Education's National Diffusion Network (NDN) and its Joint Dissemination Review Panel (JDRP) could be productive. The panel reviews project data to determine if the outcomes of a project are sufficiently validated to warrant national dissemination through NDN. Encouraging greater use of NDN could have the

positive effect of directing law-related education evaluators' attention beyond questions of only local significance.

The American Bar Association's YEFC might even consider establishing its own Law-Related Education Dissemination Review Panel. It would, of course, have to be set up to function independently of YEFC--with reviews done by law-related education research-evaluation experts according to established criteria, and with no direct YEFC participation--in order not to endanger the ABA's present (and, I believe, correct) posture of avoiding the endorsement of any particular law-related education program. At the same time, the contribution to law-related education could be considerable.

Those in law-related education might also urge that the regulations for the Department of Education's Law-Related Education Program be amended to specifically include the collection of validation data beyond local evaluation efforts in Implementation Projects, especially beyond the first year or two of operation.

Of course, traditional research approaches should not be used as the basis for setting standards for law-related education product-project validation. The standards might reflect concerns from the evaluation field with going beyond the assessment of student outcomes to, for example, the documentation of implementation conditions[3] and of other intended and unintended results with students, teachers, the school, the community. The use of standards of educational significance instead of statistical significance (Larkins & Shaver, 1972; Shaver, 1980), explicit efforts to

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[3]This documentation is similar to what educational researchers refer to as verifying that a treatment occurred and what its critical dimensions were (Shaver, 1964; Leonard & Lowery, 1979), a procedure that is not very frequently followed--which is another reason for the unproductivity of educational research.

replicate findings, and the involvement of teachers in validation-research efforts are among the non-traditional approaches that might be encouraged.

The American Bar Association, through YEFC, might play other roles in the encouragement of productive research, perhaps guided by a Research Advisory Group to help establish both substantive and methodological research priorities in the field. The publication of the Research Advisory Group's carefully considered recommendations could itself have a positive influence on research in law-related education. Beyond that, YEFC might establish a Research Awards Program in which annually one or two of those who report studies in two or more categories--e.g., the outstanding doctoral dissertation in law-related education, the outstanding published research report in law-related education--would receive a cash award and special recognition--such as a plaque, mention in YEFC publications, perhaps even a reception at the annual meeting of the National Council for the Social Studies and/or the American Educational Research Association. The Advisory Research Group could establish priorities for research--e.g., replications of certain key studies, systematic integration of research relevant to law-related education, the development and demonstration of means of establishing and assessing educational significance--with award winning studies to be those that address, with excellence, those priorities.

A particular substantive matter that might receive attention through the Research Advisory Group, and the Research Awards Program in particular, is the educational theory of John Dewey, which seems to be so closely related to the intuitive approaches of law-related education projects. Can Dewey's thinking be reformulated in the specific context of law-related education and used as a productive basis for program development and for building a cumulative body of research with utility for those who must make

curricular and instructional decisions in law-related education? That question, I believe, deserves careful consideration.

There are, of course, other research activities that YEFC, the Department of Education's Law-Related Education Program, or others interested in law-related education might organize or encourage. Careful thought might be given to a research component for LRE Regional Conferences to encourage the bridging of evaluation and research in the context of the R&D concern for not only meeting local needs but determining usefulness and obtaining use beyond the local setting, and for the broader knowledge-building purpose of probing the extent to which results can be generalized beyond specific populations and settings. The possibilities for publications exploring research options and for reporting research results in law-related education should also be considered, along with the possibility of carefully planned conferences on such topics.

No one should hold out the hope that the situation in educational research will be reversed in the next five to ten years--even in law-related education--so that substantial knowledge with clear educational implications for teachers and others in specific educational settings will begin to emerge. Change in establishments does not come easy. It may be possible, however, to capitalize on the newness of law-related education as a field to generate concern with the epistemology of human behavior and the attendant implications for research in law-related education. Mobilization of that concern could begin to lead to research reports that will do a better job of sensitizing, provoking, stimulating, and informing judgments by those who shape programs in law-related education.

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